PERSPECTIVES ON ISSUES BEYOND THE STANDARD MODEL*

GORDON KANE
Michigan Center for Theoretical Physics
University of Michigan
Ann Arbor, MI 48109

In this opening talk I first describe how we are entering a data-rich era, and what clues we might soon have to physics beyond the SM (on a time scale of several years). Then we turn to a number of the basic issues we hope to explain, and discuss what level of theory may address them, the SM or supersymmetric SM, or string theory, or "other". Next we consider some of the large number of fine-tunings we seem to have, and how they may be clues, focusing on how the fine-tuning of M_Z and m_h suggest the MSSM needs to be extended as the low scale theory, and on how flavor physics may be a powerful probe of string theory. Finally we examine using benchmark models to study all these issues.

We seem to be in a remarkable time for fundamental physics. Perhaps finally the fundamental questions are now scientifically formulated and are research problems. The parameters of the universe have mainly been measured, and now need explaining. There are good ideas about dark matter and dark energy and the matter asymmetry and neutrinos. Quarks and leptons are almost certainly the fundamental constituents – they may be described as strings or something else, but they are still quarks and leptons. Gauge theories imply the forces. It is not that all the explanations exist, but arguably we finally know what needs explaining.

There is a possible framework: M-theory $\Leftrightarrow$ string theory $\Leftrightarrow$ 4D field theory + gauge groups + high scale supersymmetry + quarks and leptons $\Leftrightarrow$ low scale effective theory, the supersymmetric Standard Model and cosmology. There are still gaps, and it could fail, but it could succeed.

We are entering a data rich era. Many experiments and facilities have gotten underway recently or will soon after a decade or more of design and

*opening plenary talk, to appear in the proceedings of susy 2003, held at the university of arizona, tucson, az, 5-10 june 2003

arranging funding and construction. They include b-factories, cold dark matter detectors, the upgraded Tevatron, ν properties experiments, rare decays, $g_\mu - 2$, proton decay, WMAP, SDS, LHC, etc. We can expect to have ideas greatly focused and constrained by data beyond the SM.

Having a framework to organize thinking is very important, but it is unlikely (though it would be welcome) that major progress will come from top-down study alone. What clues do we already have to physics beyond the SM?

◦ We have known of the matter asymmetry for a long time. There are several approaches that seem to be able to explain how the universe can evolve from an original matter-symmetric burst of energy to the observed symmetry. All require physics beyond the Standard Models of particle physics and cosmology as they are normally formulated. The problem is to distinguish among them, which should be doable based on combinations of theoretical and phenomenological analysis.

◦ We have long known that non-baryonic dark matter exists, and forms about 20% of the universe. Such matter was predicted by supersymmetry and by axions before it was settled from astronomy that non-baryonic matter was needed. The SM cannot provide the dark matter.

◦ Neutrino masses require physics beyond the SM as well, because they require a new mass scale. If the smallness of neutrino masses is from the favored see-saw mechanism, the new scale must be a high one and the underlying theory must be a supersymmetric one to stabilize the hierarchy.

These clues point in certain directions, though of course not uniquely or clearly. Serious approaches should address them.

The clues we have just listed could all be explained by short distance or cosmological phenomena. That is likely for neutrino masses, and very unlikely for dark matter, but we cannot be sure. Some kinds of data could guarantee that there is new physics at the electroweak scale. The main possibilities for such a guarantee soon are the following.

◦ For large $\tan\beta$ the decay $B_s \to \mu\mu$ could have a branching ratio large enough to be observed at the Tevatron even with its poor luminosity, and the LHC is better. In the SM it is too small to be observed, so a signal is necessarily new physics, and implies superpartners that are light enough to be produced at the Tevatron and copiously at LHC.

◦ The time-dependent CP asymmetry in $B \to \phi K_s$ is predicted in the SM to equal $\sin 2\beta$ measured in $B \to \psi K_s$ since the CPV in both arises from the initial B mixing. At present data from Belle suggests they are not

equal. If that is confirmed, again the deviation must be due to particles that are observable at the Tevatron and LHC.

◦ Another good window is the CP asymmetry in $B \to s\gamma$, which is quite small in the SM but need not be small when the SM is extended.

◦ It has long been known that $g_\mu - 2$ is very sensitive to supersymmetry breaking since it vanishes in the supersymmetry limit. Both data and theory have improved since the first suggestions a few years ago that a deviation from the SM may occur here. At present the evidence leans toward a significant deviation, but some of the theory arguments need to be confirmed. Such a deviation also could only arise from virtual particles that could be directly studied at the Tevatron and LHC.

◦ The HEAT collaboration has reported an excess of cosmic ray positrons with energies that could arise from WIMP annihilation, and are consistent with neutralino LSP annihilation, particularly with higgsino or wino type LSPs. These would have to be produced by non-thermal mechanisms to give the relic density since they annihilate well and in thermal equilibrium their number is too small to produce the observed relic density. This is presently the only direct signal for dark matter. Experimentally it is robust, having been observed in several balloon flights of particle physics detectors, with systematics that changed among the flights, but the background of cosmic ray positrons is apparently not well enough understood to be confident this is a signal of unexpected new physics.

◦ A number of cold dark matter detectors are now taking data and could report a signal soon.

◦ Improved experiments for electric dipole moments (EDMs) are or soon will be taking data. EDMs violate CP and are too small in the SM to be observed, so any signal is physics beyond the SM.

◦ The MiniBoone neutrino oscillation experiment at Fermilab should report data in 2005 that settles whether there are three independent neutrino mass differences. If three are indeed needed then sterile neutrinos (without Standard Model gauge interactions) must exist, and the implications for physics beyond the SM are profound. Many sterile neutrinos exist in models, but very special circumstances are needed for them to have typical neutrino masses and to mix significantly with the normal neutrinos.

◦ Proton decay is too small to observe in the SM, so a signal would be an exciting proof of new physics, presumably a grand unified theory.

◦ In the SM lepton flavor violating decays such as $\mu \to e\gamma$ or $\tau \to \mu\gamma$ are forbidden, but they occur in all extensions of the SM. Tau decays are studied at B factories, with new levels of sensitivity as the luminosity increases.

Improved experiments for studying μ decays are underway.

∘ The Tevatron can produce heavier particles than any previous facility. Its luminosity has not reached the levels it could have, so the event rate may be too small for signals to be seen over backgrounds, but signals may occur in the next few years. LHC will be a factory for new particle production, for superpartners and higgs bosons. Signals will be observable, but interpretation may be difficult.

One should also keep in mind that what constitutes "data" can be rather subtle, and often not noticed until thinking is ready for it. For Newton that the moon stays in orbit was crucial data, implying there must be a force acting on it toward the center of the earth. The flat old universe was important data that supported or suggested inflation. Today several non-standard pieces of data exist and have powerful implications that are sometimes ignored:

∘ the hierarchy problem, that generally in a quantum theory the Higgs boson mass will have Planck scale quantum corrections that imply it will be raised to the Planck scale. Then all masses proportional to it are also raised to that scale, the lepton and quark and W,Z masses.

∘ LEP and other precision data found no significant deviations from SM predictions at the 0.1-1% level. In quantum theory whatever effects allow the hierarchy to be maintained between the Planck scale and the electroweak scale that characterizes the actual masses will enter into all observables, so such effects must be weak and decoupled for general reasons given the LEP and other precision data,

∘ since a Higgs boson was not discovered the simple and elegant interpretation of electroweak symmetry breaking and perhaps gauge coupling unification is showing some fine-tuning that raises important questions – we will return to this below. Similar fine-tuning issues arise from the absence of electric dipole moments for electrons and quarks and from the absence of flavor changing neutral currents.

Another way to think about issues is to ask where various important questions can be addressed. Note we are only asking where the questions can be *addressed,* rather than answered – we know lots about where questions can't be answered, but not so much about where in the theory they will actually be answered. By addressed I mean not just incorporated, but actually explained in terms of more basic structures. For example, the SM can incorporate 3 families, and some CP violation, but it does not explain

the origin of these. Another way of thinking of it is that addressing the question means that the associated physics would have arisen or led to the phenomenon even if we did not already know it was there. It's fun to make a table to help think about where various issues are addressed.

Table 1.

QUESTION	SM	SSM	String Theory	Other
what is matter	$\surd$			
what is light	$\surd$			
what interactions give our world	$\surd$			
what stabilizes m_{pl}/m_W		$\surd$	$\surd$	?
gauge coupling unification		$\surd$		
explain EWSB, the Higgs mechanism		$\surd$		?
how is supersymmetry broken			$\surd$	
is there a grand unified theory			$\surd$	
proton decay		$\surd$	$\surd$	
what is the origin of flavor physics			$\surd$	
values of q,$l^{\pm}$ masses			$\surd$	
values of neutrino masses			$\surd$	?
physics of μ			$\surd$	
R-parity conservation			$\surd$	
cold dark matter		$\surd$		$\surd$
value of tanβ		$\surd$	$\surd$	
weak CPV			$\surd$	
strong CPV			$\surd$	?
baryogenesis		$\surd$		
what is the inflaton		$\surd$	$\surd$	
cosmological constant is small		$\surd$	$\surd$	
what is the dark energy		$\surd$	$\surd$	
what are quarks and leptons			$\surd$	
what is electric charge			$\surd$	
how does space-time originate			$\surd$	
how does the universe originate			$\surd$	
why does quantum theory give the rules			$\surd$	

Here SSM is Supersymetric Standard Model. We don't know if string theory addresses all the issues we assign to it, but we can be hopeful. Often

people say too little is known about string theory to make progress in answering major questions about the real world like these. We know that string theory and string theology exist, but do string cosmology and string phenomenology really exist? Perhaps it is better to approach it the opposite way – if string theory is to be relevant it is necessary to begin to do string cosmology and string phenomenology, to try to work on these issues.

The column "other" is pretty empty. Large extra dimensions and other approaches of recent years have not done well at actually addressing the issues in the table. Perhaps they address the hierarchy problem, but no more successfully than supersymmetry – in one case one must assume the large dimensions are of order the weak scale, and in the other that superpartners are of order the weak scale. Once that assumption is made the supersymmetric approach then predicts without additional assumptions both gauge coupling unification and radiative electroweak gauge symmetry breaking, while non-supersymmetric approaches explain nothing additional. Some can also deal with electroweak symmetry breaking, but only with additional special assumptions such as boundary conditions. Thus I will stay in the context of the supersymmetric Standard Model and not discuss alternatives.

To deal with the question of whether too little is known about string theory to make progress it helps to recall how little data and theory was needed to formulate the Standard Model. Basically what was known experimentally when the SM was successfully formulated was that there were quarks, 2 neutrinos, V-A currents, parity violation (chiral fermions in modern language), weak interactions were weak, the hadron spectrum, and early scaling in deep inelastic scaling. One can argue that we have information of similar quality today, as exemplified by the questions we can ask in the table. Some theoretical structures were also known, gauge theories, the Higgs mechanism, the renormalizability of the electroweak theory, and asymptotic freedom. Many theoretical questions, particularly non-perturbative ones, could not be answered, but did not prevent basically formulating the theory. Perhaps it is optimistic to think we are in a similar situation today, but it is at least defendable that the concepts and frameworks exist for clever physicists to make major progress in addressing the issues of the table.

One area that should be emphasized is flavor physics. The SM and supersymmetric SM nicely accommodate flavor physics. They do not require or explain it. While string theory has not yet solved flavor physics problems, it does address them in the sense we described above. String theories in ten dimensions have properties that can lead to flavor physics in

the 4D world, to quarks and leptons and a few replicating families, and to Yukawa matrices in the superpotential that may explain the masses. If we only knew today that the world we see was made of electrons and up and down quarks, string theory would force us to think about whether other particles existed, in families, even though understanding is not yet deep enough to call the existence of families a crucial prediction. Again one can turn it around. Flavor physics may be the area of particle physics most directly related to string theory, the area where data may most directly point to the structure of string theories. String theories have U(1) and perhaps other symmetries that may be the symmetries needed to define flavor and to understand fermion masses.

Another area that may be providing clues to point beyond the MSSM to the actual low scale theory, or to special properties of the MSSM, is several fine-tunings that are increasingly apparent as data improves. By "fine-tunings" I mean phenomena that need explaining. Lots of quantities have basically natural values, and others do not. Given M_W, M_Z has a natural value. It would be nice to explain the value of θ_W that relates them, but there is no sense that there is a fine-tuning here. If fermion Yukawa couplings all had values either of order unity or a few per cent there would be no sense of fine-tuning, since a theory that gave tree level Yukawas of order unity or zero is reasonable, and occurs in some forms of string theory, and higher order terms all of the same order is reasonable. But the double hierarchy of fermion masses, with the heaviest member of the families varying by two orders of magnitude, and masses in each family varying by over an order of magnitude, requires explaining. Similarly, if Higgs boson masses had been below about 100 GeV they would have been natural, but if they are really over 110 GeV they need special explaining.
It is worth listing fine-tunings since they can be seen as clues to the underlying theory.

∘ Why is the cosmological constant so small?
∘ Why is $\Omega_{DE} \sim \Omega_{DM} \sim \Omega_B$?
∘ Why is strong CP violation so small?
∘ The Higgs hierarchy problem.
∘ In supersymmetry, the μ problem.
∘ M_Z? In supersymmetry the natural scale for the Z mass is that of the soft parameters, a few hundred GeV or more.
∘ m_h? The higgs mass is exponentially sensitive to soft parameters in supersymmetry.

∘ The natural value of tanβ is of order unity. There is some evidence it may be large, which requires fine tuning.

∘ In supersymmetry flavor changing neutral currents are naturally much larger than observed ones.

∘ Why is $m_e \ll m_t$?

∘ If neutrino masses are hierarchical why are they so different?

Most of these are of course well known, and it is familiar to wonder what they are telling us. One reason to encourage string phenomenology is that all of them can be addressed from string theory, so it is easier to ask there what sort of clues they are providing. The allowed region of MSSM parameter space is actually quite small, and may be telling us a great deal about the underlying theory.

I want to focus briefly on two of them, M_Z and m_h and the connection between them, because more recent data has exacerbated the situation here. What do we know about the supersymmetry soft breaking parameters? They have to be of order the TeV scale in order to eliminate the hierarchy problem, but that is not very precise. Both gauge coupling unification and radiative electroweak symmetry breaking work qualitatively, and depend on the same soft mass parameters plus μ, once the hierarchy problem is solved. But we can examine them more carefully. A way to think about them is to recognize that the only relation we have that links the supersymmetry soft breaking terms to a measured number comes from explaining the Z mass with radiative electroweak symmetry breaking,

$$M_Z^2 \approx -2\bar{\mu}^2 + 6\bar{M}_3^2 + ...$$

where the dominant terms on the RHS are shown, M_3 is the SU(3) soft breaking mass, the bars above μ and M_3 mean they are evaluated at the high scale, and the coefficients depend on various kinds of assumptions but some such relation is robust. Existing data on chargino production requires that $\bar{\mu}$ be larger than about M_Z, and that M_3 be even larger. Thus this relation requires differences of large numbers to “explain” M_Z. A number of ways out of this, including lowering the high scale, looking for relations between $\bar{\mu}$ and $\bar{M}_3$, extra matter, etc, do not change the problem that the LHS seems to be small compared to the RHS.

This problem has been around for a long time, but it has become more serious as ways out were examined and found not to work. It has become worse recently as lower bounds on the Higgs mass have tightened, because

m_h is also calculable in terms of soft parameters. The tree level higgs boson mass is given by

$$m_h^2 \leq M_Z^2 \cos^2 2\beta$$

in the MSSM, so any of m_h above M_Z must come from radiative corrections involving soft parameters. To get m_h up to 115 GeV (remember they add quadratically) requires large soft parameters, in particular large M_3, which worsens the situation for M_Z! There is also great sensitivity here. For example, increasing m_h from 112 to 115 GeV doubles the needed M_3!

What does this imply? One possibility is that the higgs mass is actually lighter than 115 GeV but because the cross section is suppressed or the signature is non-standard no signal was seen at LEP. This is possible, but the kinds of models needed for it to happen are not so attractive that any seem worth pursuing in the absence of confirming data. Another possibility that seems well worth taking seriously is that the correct *low scale theory* is not the MSSM but some extended theory. Note that only extensions that significantly change the higgs sector, so as to modify both the calculation of M_Z and of m_h, can be relevant. Both are very sensitive to the smallness of the coefficient of the fourth power of the higgs field in the higgs potential, so modifying that is a way to focus. That supersymmetry fixed that coefficient in terms of gauge couplings was a great success of supersymmetry, but the result is $\sim (g_1^2 + g_2^2)/8 \ll 1$ rather than ~ 1 as suggested by the data. Of course one could say that the needed fine tunings in supersymmetry suggest that supersymmetry is not the correct approach, but all other approaches are so much more fine tuned in many ways that we clearly want to improve on minimal supersymmetry rather than reject it.

Another fine-tuning that is part of flavor physics is the issue of the phases of the soft parameters. They are complex masses, and introduce a number of new phases, which lead to a number of new CP violation effects. Most dramatic are electric dipole moments for electrons and quarks, that are expected to be one to two orders of magnitude larger than the current limits. One could interpret this to be telling us that the relevant soft phases are real, in which case we would have a significant clue about the underlying theory. The soft phases depend on the superpotential and the Kahler potential directly from the string theory, and on supersymmetry breaking, which could be from F-term vevs. So far, however, no principle or argument has been found that implies the phases should be small. Phenomenologically,

the phases that enter so far could be small from effective cancellations that appear natural in the high scale theory but seem fine-tuned in the low scale theory (examples are known). If so non-zero EDMs would have to appear as experiments improve a little more, and then the results would point us in quite a different direction about the phase structure of the high scale theory.

So how should we proceed? There is a great deal of interesting and exciting work to do. First of course it is necessary to get as much experimental and phenomenological information about the soft breaking Lagrangian as possible, the soft masses and μ and the soft phases, from colliders and b-factories and rare decays and EDM experiments and dark matter experiments. This is difficult because the information from all of these depends on a number of soft parameters at once, and we never can have enough observables to invert the equations to measure a single soft parameter, or $\tan\beta$, until we have an electron linear collider – and that is at least 17 years off if one tracks through the time line required by funding and site and construction issues. So considerable clever thinking is needed by experimenters and theorists to untangle the low scale theory. Further, even if we can learn some or most of the low scale theory there are many obstacles to extrapolating to the high scale effective Lagrangian. These include possible intermediate scale matter, not knowing the several high scales that may exist, not knowing the full gauge group, possible D' terms that affect scalar masses, extra Higgs and neutralinos, and more. Again, considerable challenging work is needed, here particularly by phenomenological theorists, to learn to make progress. Finally even after we learn much of the high scale Lagrangian we have to recognize its implications for connecting to an underlying string theory, what it is telling us about where the world is in the M-theory amoeba.

A very good way to study all these issues is to study "benchmark" models. Basically one can begin with a string theory and start to work out what it predicts for the superpotential, the Kahler potential, and the soft breaking Lagrangian that together determine the properties of the observable particle physics world. Of course today assumptions must be made to proceed at a number of stages, so one is constructing a model. Doing so is very good for improving one's understanding of the theory at many levels. Once a model is obtained one can simulate what sort of discoveries of physics beyond the SM would be made. Then one can pretend one only has those phenomena, with experimental errors, and try to reconstruct the high scale theory one

knows one started from. If the model has intermediate scale matter one can see how to learn that from the low scale data. Eventually a number of clever people could learn how to proceed with real data as we learn that. One can sometimes skip steps – if one plots graphs of relations among several low scale quantities one finds that various high scale theories lead to different relations in characteristic ways, as an example of how innovative studies could teach us techniques to overcome obstacles. I have described using benchmark models to study collider physics and some aspects of the soft breaking Lagrangian, and a similar approach could be used to relate flavor phenomena and string theory. Of course one could wait until there is data, but it is hard to recognize when enough incomplete data has accumulated to make progress, while if we carry out these studies now we may find progress earlier than we expect.

Acknowledgments: I would like to thank the organizers, and particularly Keith Dienes, for a stimulating meeting and kind hospitality. I appreciate very much a number of discussions and collaborations, particularly with Lisa Everett, Steve King, Joe Lykken. Pierre Binetruy, Brent Nelson, Ting Wang, and Liantao Wang, on the topics here. Since this is an introductory talk for the meeting and covers many topics, rather than a review, I have chosen to rigorously not provide references not only to the field broadly since that would have required hundreds of references, but also to specific results. I apologize to those who would have been referenced, and particularly to my collaborators. The reader can easily find the relevant references to pursue each topic.